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PROFESSOR CAYLEY, President, in the Chair.

Henry Pratt, Esq., 18 Preston Street, Brighton;
H. C. Levander, Esq., 376 Wandsworth Road; and
Geo. Mathus Whipple, Esq., Observatory, Kew,

were balloted for and duly elected Fellows of the Society.

The Second Part of a Memoir on the Development of the Disturbing Function in the Lunar and Planetary Theories. By Prof. Cayley. (Abstract.)

The present communication is a sequel to my paper, "The First Part of a Memoir on the Development of the Disturbing Function in the Lunar and Planetary Theories," *Mem. R. A. S.*, vol. xxviii. (1859), pp. 187–215, and I have therefore entitled it as above, but it in fact relates only to the Planetary Theory. In the First Part, I gave in effect, but not explicitly, an expression for the general co-efficient D (j, j') in terms of the co-efficients of the multiple cosines of θ in the expansions of the several powers $(r^2 + r'^2 - 2\,r\,r'\cos\theta)^{-s-\frac{1}{2}}$, or say $(a^2 + a'^2 - 2\,a\,a'\cos\theta)^{-s-\frac{1}{2}}$; viz., at the foot of page 208 I speak of the term involving $\cos(j\,\mathrm{U} + j'\,\mathrm{U}')$ as having a certain given value; the term in question is D $(j, j')\cos(j\,\mathrm{U} + j'\,\mathrm{U}')$; and consequently the expression for D (j, j') is

$$\mathrm{D}(j,j') = \Sigma \frac{\Pi_1(x-\frac{1}{2})}{\Pi x}\eta^{2x}\Sigma\,\mathrm{M}_x^{\vartheta}\,\mathrm{R}_x^{\vartheta};$$

the omission was however a material one, inasmuch as this expression for the general co-efficient serves to connect my formulæ with Leverrier's development, *Annales de l'Obser. de Paris*, t. I. (1855) pp. 275–330 and 358–383, and I resume the question for the purpose of supplying it.

On the Proposition 38 *of the Third Book of Newton's Principia.*
By I. Todhunter.

In this Proposition Newton undertakes to determine the figure of the Moon. The Moon is supposed to be a homogeneous fluid, drawn into an oblong shape by the disturbing action of the Earth; the rotation of the Moon on its axis, and the revolution of the Earth with the Moon round the Sun, are disregarded. The Proposition is connected by Newton with one that precedes, in which the Earth is supposed to be fluid, and to be similarly acted on by the Moon.

Let M denote the mass of the Earth, and m the mass of the Moon. Suppose that the Earth is a homogeneous fluid, and that, owing to the disturbing action of the Moon, it assumes the form of an ellipsoid of revolution round the major axis. Let B denote the minor axis, and B + H the major axis, where H is supposed small compared with B; let k denote the distance between the centres of the Earth and of the Moon. Then we shall have approximately

$$H = \frac{15}{4} \cdot \frac{m}{M} \cdot \frac{B^4}{k^3}$$

Newton does not explicitly give this formula; but his numerical results in Propositions 36 and 37 appear to have been deduced from it; and it can be obtained readily in the same way as Newton employed elsewhere: see Whewell *on the Free Motion of Points*, third edition, page 202. The formula was obtained by D. Bernoulli in his prize essay on the Tides, Chapter IV., Article 8.

In like manner let b denote the minor axis, and $b + h$ the major axis of the ellipsoid of revolution, which the Moon becomes when disturbed by the Earth; then

$$h = \frac{15}{4} \cdot \frac{M}{m} \cdot \frac{b^4}{k^3}$$

Hence by division we obtain

$$\frac{h}{H} = \left(\frac{M}{m}\right)^2 \left(\frac{b}{B}\right)^4 \qquad (1)$$

Newton, however, in his Proposition 38, instead of (1), makes

a statement in words which is equivalent to the following formula :—

$$\frac{h}{H} = \frac{M}{m} \cdot \frac{b}{B} \qquad (2)$$

Newton himself gives no reason for the statement.

In the *Astronomiæ Elementa*, by David Gregory, published at Oxford in 1702, a proposition is ennunciated, which is substantially equivalent to the formula (2), and is supported by a folio page of general reasoning, which is, however, altogether unsatisfactory ; see page 387 of that work.

In the Jesuits' edition of the *Principia* a note is given, which is founded on the process of David Gregory, and is also quite inadmissible.

Newton's error was pointed out with some emphasis by Paul Frisi ; see pages 174 and 186 of his *De Gravitate universali*, 1768, page 135 of the second volume of his *Cosmographia*, 1775, and page 170 of the third volume of his *Opera*, 1785.

If the error had been generally admitted, and the correction adopted, it would of course have been superfluous to recur to the matter ; but so far as I have been able to find, no attention has been given to it. Writers and commentators have sometimes touched on Propositions 36 and 37 in such a manner that we might naturally have expected an allusion to Proposition 38, if only by way of warning ; but nothing is said about it.

For a more decisive case, we may refer to *Robison's Mechanical Philosophy*, 1804. On page 512, he speaks of Frisi's *Cosmographia* as a very masterly performance on the subject of the figure of the planets ; and in his page 517, he alludes to Newton's Proposition III. 38, without any remark as to the criticism on it by Frisi.

But the most remarkable case is that furnished by the book of Dr. Whewell's, which I have already cited. On page 206, Newton's statement is reproduced without any indication whatever of its untenable character. This seems very strange, when we reflect that Dr. Whewell paid great attention to Newton's work, and also to the Theory of the Tides, with which the Proposition has some connexion.

Lord Brougham touches on the subject in his *Analytical View of Sir Isaac Newton's Principia;* he says on his page 295, "The tide in the Moon caused by our Earth is to the tide in our sea caused by the Earth, as the mass of the Earth to the mass of the Moon." I presume that on the second occasion where the word *Earth* occurs, it is a misprint for *Moon ;* and then the statement amounts to the formula,

$$\frac{h}{H} = \frac{M}{m}$$

This agrees neither with (1) nor with (2), and is of course quite wrong.

Laplace has considered the problem of the figure of the Moon in his *Théorie du Mouvement et de la figure elliptique des Planètes.* He takes into account the rotation of the Moon on its axis ; but otherwise his solution is consistent with Frisi's result, and not with Newton's ; see page 115 of the work. He does not mention either Newton or Frisi.

The Proposition to which this note relates is of no practical importance ; but even in pure theory it is well to be accurate and consistent : I have therefore thought it would be useful to draw attention to a curious error which has indeed been corrected, but certainly not obliterated.

The Source of the Solar Heat. By Maxwell Hall, B.A.

Let us suppose that the mass of the Sun is slowly, but continually, contracting ; then, in consequence of the enormous mass subjected to this contraction, an enormous amount of heat will be developed, and it will be found that the rate or amount of contraction necessary to produce the amount of heat radiated by the Sun into space, is so remarkably small, that ages must elapse before the effect of this contraction can become visible to us at our comparatively great distance from the Sun.

In order to show that this is the case, let one foot and one second be taken as the units of space and time, and suppose that each unit of volume of the Sun's mass contracts by the same amount in the same time, so that if z_0 be the linear contraction of the Sun's radius r_0 in one second, and if z be the contraction of any other length r, measured from the centre, and for the same duration of time, then $\frac{z}{z_0} = \frac{r}{r_0}$. The effect of this contraction may thus be compared to a series of intermittent pulsations, acting throughout the whole of the mass, and tending to diminish the volume. Let g_0 be the force of gravity at the surface of the Sun, and let g be the force of gravity at any point within the Sun's mass considered homogeneous, whose distance is r from the centre ; then $\frac{g}{g_0} = \frac{r}{r_0}$. Again, let ϱ be the mean density of the Sun's mass, so that the weight of any thin concentric shell, whose radius is r and thickness δr, will be $4\pi g \varrho r^2 \delta r$; and, since every unit of mass in this shell falls through z feet towards the centre in a second of time, $4\pi g \varrho z r^2 \delta r$ will be the kinetic energy generated and destroyed every second by this shell alone ; and therefore $\int_0^{r_0} 4\pi g z r^2 dr$ will be the whole kinetic energy destroyed every second of time, and we proceed to find the corresponding amount of heat evolved.

$$\text{Now} \int_0^{r_0} 4\pi g \varrho z r^2 dr = \frac{4\pi g_0 \varrho z_0}{r_0^2} \int_0^{r_0} r^4 dr = \frac{4}{5} \pi g_0 \varrho z_0 r_0^3,$$

and this is the kinetic energy destroyed by the fall of a weight $\frac{4}{5}\pi g_0 \varrho r_0^3$ through a height of z_0 feet, or by the fall of a weight $\frac{4\pi g_0 \varrho z_0 r_0^3}{5 \times 1390}$ through a height of 1390 feet; but the fall of one pound avoirdupois through a height of 1390 feet generates sufficient heat to raise one pound of water through 1° centigrade, or it generates one thermal unit; hence by expressing $\frac{4\pi g_0 \varrho z_0 r_0^3}{5 \times 1390}$ in foot-pounds we shall get the number of thermal units generated every second of time. Now $g_0 \varrho$ is the weight of a cubic foot of the Sun's mass at the surface, and since

g_0 = 27·20 times the force of terrestrial gravity, and

ϱ = 1·43 times the density of water, therefore

$g_0 \varrho$ = 27·20 × 1·43 times the weight of a cubic foot of water at the surface of the Earth. But a cubic foot of water weighs 62·5 pounds, so that $g_0 \varrho = 2431$ pounds.

Therefore $\frac{4\pi z_0 r_0^3 \times 2431}{5 \times 1390}$, when both r_0 and z_0 are expressed in feet, will give us the number of thermal units generated every second of time.

Now, it has been found by observation that the heat emitted by the Sun to the Earth is sufficient to melt a sheet of ice whose thickness is 0·01093 inches, when exposed perpendicularly to the Solar rays for one minute (Sir John Herschel, *Meteorology*), due allowance having been made for the heat absorbed by the atmosphere; and, therefore, in one second, a sheet of ice whose thickness is 0·0000152 feet will thus be melted; so that, if a be the mean distance of the Earth from the Sun, expressed in feet, a spherical shell of ice, whose radius is a and thickness 0·0000152 feet, will be the volume of ice in cubic feet melted every second by the whole of the radiant Solar heat. But one pound of ice has a volume equal to $(0{\cdot}2584)^3$ cubic feet, therefore $\frac{4\pi a^2 \times 0{\cdot}0000152}{(0{\cdot}2584)^3}$ will be the weight of the ice in pounds thus melted.

Again, in order to melt one pound of ice, 79·25 thermal units are required; and thus the whole Solar heat evolved in one second of time is equal to $\frac{4\pi a^2 \times 79{\cdot}25 \times 0{\cdot}0000152}{(0{\cdot}2584)^3}$ thermal units.

Now, by equating the heat generated to the heat evolved in one second, we get

$$\frac{4\pi z_0 r_0^3 \times 2431}{5 \times 1390} = \frac{4\pi a^2 \times 79{\cdot}29 \times 0{\cdot}0000152}{(0{\cdot}2584)^3},$$

and the contraction z_0 is therefore only 0·000004079 feet in one second, or 129 feet per annum; and, as we have already said, ages must elapse before the effect of this contraction can become

visible to us, whether we compare direct measures of the Solar disk or observed periods of axial rotation.

The contraction, therefore, is so small, that as much allowance can be made for the assumptions introduced above as may be thought necessary, without altering our general conclusion in the slightest degree, namely, that the source of the Solar heat and light is connected with the mechanical theory of heat by means of the contraction of the composing mass.

The application of this theory to other bodies is almost without limit; the Earth has contracted, and has stored up a corresponding amount of heat in the non-conducting rocks and soils; the stars, by their intrinsic brilliancy, indicate the operation of the force of gravity upon contracting matter; the nebulæ afford examples of the commencement of this operation; and periodical variations in light now become perturbations, the effect of disturbing masses in motion, producing endless changes subject to the great principle known as the conservation of energy.

January 1872.

On the Insufficiency of Existing National Observatories.

By Lieut.-Col. A. Strange, F.R.S.

The Astronomer Royal, in a recent paper* on the "Proposed devotion of an Observatory to observation of the phenomena of Jupiter's Satellites," introduced his remarks in these words :— "The position which the Royal Astronomical Society holds in the astronomical world may well justify it in employing its judgment and its influence in the direction of astronomical enterprise exterior to its own body."

Adopting Mr. Airy's plea, I now propose to invite the attention of the Society to some further consideration of the question he has raised, as to the sufficiency of existing National Observatories.

These establishments were organised at a time when the objects of astronomical research were comparatively few, readily enumerated, and easily defined. The Royal Observatory was founded in the interests of navigation, and though that purpose has been very liberally interpreted, and much of the work done has had far higher aims, still, on the whole, it has been, for the most part, of a routine, observational character.

But within a comparatively recent period, the science of astronomy has made enormous advances in directions not contemplated when Greenwich was founded. And it has been for some time clear, that Greenwich cannot, as at present constituted, be expected to contribute systematically to the advancement of the new branch of astronomy to which I allude, namely, what is

* *Monthly Notices*, Jan. 12th, 1872, vol. xxxiii. No. 3.

now generally recognised as the Physics of Astronomy. I have spoken of Greenwich as the type, and the highest type, of National Observatories; but if we turn to the Report of the Council of the present year, which gives us the latest account of the operations of all such observatories as can be considered in any sense official, we shall find the same characteristic prevailing, namely, a nearly total exclusion of physical work.

I do not state this in order to reflect disparagingly on the proceedings in question. With respect to the Royal Observatory of Greenwich, indeed, there can be no doubt that its strict adherence for so long a period to a well-marked system of almost purely meridional observation, has conferred incalculable benefits on astronomy, and I wish to express my humble tribute of admiration of the wise self-denial which has characterised its administration, and which has been proof against the allurements of researches, the pursuit of which must have militated against the primary functions of the Institution, namely, that of laying, extending, and conserving the very foundations of the science.

Still the fact must be plainly recognised, that whilst one branch of astronomy is provided for in the most efficient and permanent manner, the other branch is left literally to chance. And the question I wish now to ask is, whether this is a satisfactory state of things; and if not satisfactory, whether the first astronomical body in the world cannot do something to amend it? I know that this question is very likely to raise a controversy as to the value of private exertions, and as to the risk of damping individual enterprise by the establishment of new and powerful official institutions. I therefore, at the outset, beg to express my great respect for the labours of those who have devoted their means and time, gratuitously, to the cultivation of science in whatever department. I should be very sorry to see the spirit of individual research, in which Englishmen exceed all other people, discouraged by the action of the community; but I have no fear of such a result. I believe that by a proper classification of objects, there will be left an ample field for private observers, even should official astronomy be very much extended.

It appears to me that this classification should be based on the principle that researches which involve long-continued, systematic labour, should be undertaken by permanent establishments. I would give, as a simple example of such researches, the study of the Sun—a study which, in all probability, will never come to an end. In such a study, well-considered measures, strictly and uninterruptedly persevered in for a long period of years, are absolutely indispensable. It is certain that we cannot count upon such extended continuity from private energy, which however supreme it may be, must die with its possessor.

On the other hand, the work which may be expected from volunteer astronomers consists chiefly in opening out new fields, and in devising new processes of inquiry. I may here again cite the same example of the Sun, the systematic study of which has

been rendered possible by the personal exertion and genius of men too well known to us all to need enumeration. But the work is now fast getting too heavy for private resources. The mapping of the Moon is another example of work which individuals have shown us how to do; but which individuals can never complete within any reasonable period.

My present object is to draw attention to the general question of the insufficiency of existing astronomical establishments, in order to elicit opinions on it. The details of any new scheme will be matters for future consideration. At present, I shall content myself with stating my opinion that permanent national provision for the cultivation of the Physics of Astronomy, is urgently needed. If the study of the Sun only were in question, that alone would, I think, justify such a measure ; for there can hardly be a doubt that almost every natural phenomenon connected with climate can be distinctly traced to the Sun as the great dominating force, and the inference is unavoidable, that the changes, and what we now call the uncertainties of climate, are connected with the constant fluctuations which we know to be perpetually occurring in the Sun itself. The bearing of climatic changes on a vast array of problems connected with navigation, agriculture, and health, need but be mentioned to show the importance of seeking in the Sun, where they doubtless reside, for the causes that govern these changes. It is indeed my conviction, that of all the fields now open for scientific cultivation, there is not one which, quite apart from its transcendent philosophical interest, promises results of such high utilitarian value as the exhaustive, systematic study of the Sun.

The present seems to me an opportune time for agitating this question, for two reasons. In the first place, it has been recently announced that the famous series of Solar photographic researches conducted by Messrs. De La Rue, Balfour Stewart, and Loewy, have been brought by circumstances to a close. The changes daily in progress on the Sun's surface are now therefore no longer systematically depicted by photography anywhere in the British empire. The results obtained, as recorded by the three above-named astronomers in the *Transactions* of the Royal Society, are of such striking interest and importance, that any prolonged loss of continuity in the series will be a most deplorable circumstance. No time, therefore, should be lost in averting such an evil.

In the second place, it is, no doubt, generally known, that a Royal Commission, of which the Duke of Devonshire is Chairman, has been for about two years engaged in a twofold inquiry into "Scientific Instruction and the Advancement of Science." The first branch of their inquiry is completed, and they are about to enter on the second, namely, the "Advancement of Science." We may feel very sure that the present generation will not see so comprehensive an inquiry undertaken a second time, and that any conclusions this Commission may now come to will govern the

principles of State action with regard to science for many years to come. The opportunity, therefore, which is now open to the Astronomical Society to make any recommendations which in the present state of the science may seem to it advisable, is one which for a long period may not again be presented to it.

On the Law of Facility of Errors of Observations, and on the Method of Least Squares. By J. W. L. Glaisher, B.A., F.R.A.S., F.C.P.S., Fellow of Trinity College, Cambridge. (Abstract.)

The *American Journal of Science and Arts* for June 1871, contains a note by Professor Cleveland Abbe, the object of which is to point out that Professor Robert Adrain, of New Brunswick, published the method of Least Squares in 1808, having been independently led to its discovery. It is well known that all the proofs that have been given of the method of Least Squares, contain, to say the least, some points of difficulty, and on this account any new investigation of the result is necessarily a matter of much interest. Although some of the investigations of the law $e^{-h^2x^2}$ are far from rigorous, still there is not one that is not of some importance, as throwing additional light on the properties of this law, so that a fresh investigation, and one moreover, by which the law, not previously known to the author, was discovered, might be suspected to be a real addition, or at all events, confirmation of the known processes. Dr. Adrain's investigation is therefore examined at some length; but such is not found to be the case; the reasoning being of so slight and inconclusive a nature as to lead one to believe that, like Legendre, Dr. Adrain first remarked the convenience of testing equations by the method of Least Squares, and subsequently endeavoured to justify this rule by the Theory of Probabilities. After noticing the way in which Legendre originally presented the method, the other investigations that have been given are examined in detail, being grouped as follows:—

1. Gauss's original investigation, including Encke's, De Morgan's, and Leslie Ellis's remarks on the principle of the Arithmetic mean.

2. Laplace's Method, including Poisson's and Ellis's Simplifications, and Ivory's Criticisms.

3. Gauss's Second Demonstration, and its connexion with Laplace's.

4. Sir John Herschel's proof, with Ellis's and Boole's Criticisms thereon.

5. Professor Tait's, and similar proofs (Quetelet's, Hagen's, &c.)

6. Donkin's Proof.

Besides the above, four investigations were given by Ivory of the method, of which only one is referred to in detail, the others having been discussed by Ellis. Memoirs by Bessel and Mr. Crofton are also referred to.

The principle of the arithmetic mean, regarded either as an axiom or a result of demonstration, is examined, and the author believes them to be untenable. Laplace's method, as extended by Ellis, is restated with some alterations, and Poisson's general theorem given in the *Connaisance des Tems* for 1827, is proved, starting at once from a multiple integral, which is evaluated by means of Lejeune-Dirichlet's principle. The awkward transition from finite to infinitesimal errors in the course of the analysis is thus avoided. The exact sense in which Laplace's system of factors gives the most probable values of the quantities to be determined, is examined in some detail, and attention is drawn to the case taken by Laplace, when the facilities $\phi_1(x)$, $\phi_2(x)$ are supposed known, and the most probable values of the unknowns are to be found; and the actual case when in effect $\phi_1(x)$, $\phi_2(x)$ as well as the most probable values, are to be found. The one case is, given the observations and weights, find the values; the other, given the observations, find the weights and values.

The method of Least Squares gives in fact only the first approximation to the true values, which, however, enables us to weigh the observations and so obtain a second approximation, and so on. A criticism like Professor Pierce's is false in principle: it may certainly be better to reject an observation entirely, than to give it a weight equal to that of the best; but a proper extension of the method of Least Squares would give an abnormal observation a very small weight. Reasons are given why Laplace's first view of the subject seems much preferable to his second (the disadvantages in which are considered), and therefore preferable to Gauss's second demonstration also. The two views of the matter taken by Laplace (in his first investigation), and by Gauss, lead to a theorem in multiple integrals, which is verified. The final result of the discussion of all the proofs of the method that have been given is, that the only correct and philosophical view of the subject is that which regards an error as formed by the aggregation of a great number of smaller errors, due to independent sources, and subject to arbitrary laws of facility, whence by Laplace's analysis (though it was not so applied by its author), we have generally the law $e^{-h^2x^2}$ for individual actual errors, and the rule of Least Squares follows at once from this law, so that if the number of observations be large, we have, as it were, a double reason for the method. The paper concludes with one or two curious conclusions that would follow if the law of facility were $e^{-m\sqrt{x^2}}$, a form which not only seems very natural, but which was actually assumed to be the true one by Laplace in one of his earlier memoirs.

The Solar Eclipse of December 12th, 1871.
By Mr. Tebbutt.

The delicate state of my health prevented me from joining the expedition for observing the total eclipse at Cape Sidmouth; but I availed myself of the instrumental appliances at Windsor, for the observation of the partial phase. It unfortunately happened that the evening of the 11th was cloudy, so that no observations could then be taken for the determination of the mean time at the Observatory. The following morning, however, broke clear and fine, and I succeeded some hours after sunrise in observing the transits of two stars of the first magnitude. These observations in connexion with others taken in the evenings of the same and the following day, served for the determination of the chronometer errors and rate. A strong N.W. wind prevailed throughout the day of the eclipse, and broken clouds passed rapidly across the sky during the forenoon. With the exception of two or three small fleecy clouds, which passed almost instantaneously over the disc, and that without appreciably obscuring it, the Sun was bright and unclouded throughout the phenomenon. There were several spots on the Sun, but they were not of a very remarkable character. A group of small spots was to be seen on the eastern limb, involved in an immense tract of faculæ. The first contact appeared to take place at $2^h\ 18^m\ 39^s{\cdot}1$, owing, however, to a momentary removal of the eye from the telescope, this phase of the eclipse must have been observed somewhat late, probably to the extent of two or three seconds. The Sun's limb was remarkably steady and sharply defined. A very small sun-spot was observed to disappear at the Moon's edge, at $2^h\ 40^m\ 44^s{\cdot}5$, and at $2^h\ 57^m$, the same spot was again seen immediately after it had emerged from the Moon's limb. The most conspicuous spot on the disc was bisected by the Moon's limb at $3^h\ 28^m\ 41^s{\cdot}9$. Up to $3^h\ 30^m$, the images were very steady, and the cusps sharply defined; there being no rounding-off of the latter at the extreme points. At this time, however, the Sun's limb began to boil, which disturbance increased rapidly at $3^h\ 36^m$. The large conspicuous spot was again bisected at $3^h\ 37^m\ 24^s{\cdot}4$, while emerging from the Moon's limb. At $3^h\ 43^m$, the limb approached very near to the large tract of faculæ, but did not pass over it. During the early progress of the eclipse, the mountain-peaks and ranges along the edge of the Moon were beautifully distinct against the solar background. I believe I never before saw them to such advantage. Towards the end of the eclipse the images became more steady. The last contact took place at $4^h\ 4^m\ 38^s{\cdot}3$. The most interesting circumstance perhaps in connexion with the eclipse was the effect which the phenomenon had on the vacuum solar thermometer, suspended freely in the Sun's rays. From the beginning of the eclipse the thermometer gradually sank till about the time of greatest obscuration, when it began to rise, and continued to do so till the end of the phenomenon. The

instrument then, owing to the decreasing altitude of the Sun, began to fall again.

As the readings of the thermometer may prove interesting, I subjoin them:—

Readings of the Solar Thermometer during the Eclipse, December 12th, 1871.

Mean Time.	Therm.	Mean Time.	Therm.	Mean Time.	Therm.	Mean Time.	Therm.
h m s	deg.	h m s	deg.	h m s	deg.	h m s	deg.
2 22 25	114·8	3 5 25	107·4	3 46 40	109·5	4 18 10	112·1
2 24 55	114·3	3 8 55	107·0	3 49 55	110·0	4 19 55	111·8
2 32 25	113·6	3 12 25	106·3	3 52 55	110·5	4 22 25	111·1
2 36 25	112·6	3 15 55	106·1	3 55 40	111·0	4 24 55	111·0
2 41 55	111·5	3 19 55	106·0	3 59 55	111·8	4 27 25	110·4
2 44 55	111·1	3 22 55	106·1	4 2 25	112·8	4 29 55	109·7
2 48 55	110·5	3 25 55	106·3	4 5 55	113·5	4 32 25	109·2
2 52 55	109·6	3 29 55	106·6	4 9 25	113·3	4 34 55	109·0
2 55 25	109·0	3 33 55	107·3	4 11 55	113·0	4 50 55	107·0
3 0 25	107·7	3 39 55	108·7	4 13 55	112·9	5 27 25	103·8
3 3 25	107·2	3 43 40	109·0	4 15 55	112·5	6 16 25	93·0

On the Aurora of February 4. By H. P. Finlayson.

As it is probable that some members of the Royal Astronomical Society saw the Aurora Borealis of the evening of Sunday last, Feb. 4th, I am anxious to record its appearance as seen here; for, should the display have been similar to an observer at some considerable distance from Dover, it may be possible thereby to obtain some faint idea at what height such an electric storm takes place. The fore part of the evening was cloudy, and no stars visible; the clouds close to the Earth seemed vapoury, or large foggy clouds, and were reflecting a blood-red colour, as from some great conflagration. One could not tell how this red light was obtained, for above them there appeared to be one large cloud that covered the whole of the sky, thus preventing the stars being seen between the openings of the lower clouds. This led me to think that the cause of this red light must be very close to the Earth, on a level with or just above the lower clouds. This, however, cannot be correct, since the phenomenon was seen in Paris and Constantinople.

Between 10 and 11 the sky was quite clear of clouds, and then the Aurora Borealis was seen in perfection,—rays of light in every direction, and all radiating from one point in the heavens. At one time there were so many streamers, and the radiation so complete, that the point of departure was pretty accurately

given; it was in the constellation *Gemini*, and between *Jupiter* and the two principal stars.

On intently watching these rays of *white* light, I found they had a slow motion in the same direction as the Earth on its axis: thus showing that the whole must have been revolving very slowly round the radiating point, taking, I should think, five or six hours to complete a revolution. Many of the rays were 60° to 70° long, and running direct from the centre. The light of the stars was very little interfered with when a streamer passed over them: I should say not at all.

Now, should you have any other description of this Aurora Borealis and the radiant point altogether different, it would give us some clue whether all this emanates from the atmosphere or above it.

At the beginning of last month I noticed a very large halo round the Moon, quite 40° to 60° in diameter, a complete circle; and yet the Moon was very little more than a thin crescent. This I thought strange and curious, and, thinking it may prove interesting, have mentioned it here.

Dover, Feb. 8, 1872.

Occultation of ζ Tauri, December 8th, 1870.

By J. Maguire, Esq.

In the last Supplementary Number of the *Monthly Notices* there is a communication from the Rev. J. Spear, from Churkrata, in India, in which the following statement occurs :—

"I am sorry I have not been able to record any observations of importance, except perhaps the error in the calculation of the occultation of *ζ Tauri*."

"December 8. Watched for the occultation of *ζ Tauri*. Greenwich mean time of apparent conjunction in right ascension of Moon and star $5^h\ 0^m\ 3^s$. Limiting parallels N 90–N 19. No occurrence. The Moon passed at least 10′ north of the star to the best of my judgment."

The geographical position given is—

Latitude	30° 42′ 4″
Longitude	$5^h\ 11^m\ 42^s$

I may add that the right ascension at conjunction is $5^h\ 0^m\ 39^s$ in the *Nautical Almanac*, and the latitude, although not marked N., I have assumed to be so.

If, then, the error above mentioned refers to what took place on the 8th December, 1870, it appears that Mr. Spear, finding

himself well within the limiting parallels, watched with confidence for the occultation of ζ *Tauri*. But his surprise may be imagined when he found that the Moon's lower limb at its nearest approach was in his estimation at least 10′ north of the star. He records the words "no occurrence," and leaves us to infer that he has discovered an important error in the *Nautical Almanac*.

If there be many observers who take it for granted that an occultation must occur at all latitudes within the limiting parallels, then I fear the *Nautical Almanac* will get a bad name.*

I have calculated the limiting parallels, which I find the same as in the *Nautical Almanac*. And a calculation for the position given above shows that the nearest approach of the Moon's centre to the star was 19′·3. The moon's lower limb was, therefore, within 4′·5 of the star. This differs materially from Mr. Spear's 10′. The question, however, is not one of proximity, but whether a limiting parallel in such a case as this is a delusion and a snare.

Norwich, Nov. 13, 1871.

Discovery of the Minor Planet (118) *Peitho.*

This planet was discovered by Dr. R. Luther, at Bilk, near Dusseldorf, on March 15, 1872. It was of the eleventh magnitude. The following elements have been calculated by Professor Theodore von Oppolzer, from observations made on March 15, 26, and April 4:—

Epoch, 1872, March 31·0, Berlin Mean Time.

L	= 160° 53′ 44″·7	
M	= 84 25 12·3	
π	= 76 28 32·4	Mean Equinox 1872·0
Node	= 47 14 25·5	
i	= 7 50 11·3	
φ	= 9 51 25·0	
μ	= 928″·402	
log a	= ·0·388181	

Sidereal revolution = 1396 days.

[* In the Explanation of the Elements of Occultations, at the end of the *Nautical Almanac*, it is stated that "by Limiting Parallels are to be understood those parallels of latitude beyond which an occultation cannot *possibly* occur. . . . Limiting parallels are useful to indicate whether, at a given conjunction of a star with the Moon, the positions are likely to produce an occultation in a given latitude, and thus to save considerable labour to the computer." Ed.]

The following ephemeris may serve for comparison with the observations:—

12h Berlin M.T.	R.A. h m s	N.P.D. ° ′	Log Δ.
April 4	11 48 22	78 50·7	
8	11 45 11	78 48·8	0·194
12	11 42 22	78 50·8	
16	11 39 55	78 54·5	0·212
20	11 37 54	79 1·9	
24	11 36 18	79 12·0	0·232

Peitho is the nineteenth minor planet discovered by Dr. R. Luther.

April 10, 1872.

Observations of the small Planets Peitho (118) *and Egina* (91), *of six new Nebulæ, and a Variable Star.* By M. Alph. Borrelly, Assistant Astronomer at the Marseilles Observatory.

(*A Letter to the President.*)

I have the honour of forwarding to you two observations of the small planets *Peitho* (118) and *Egina* (91). To these I add, in like manner, the positions of six new nebulæ, and of a variable star.

Observation of the Planet (118) *Peitho.*

	Marseilles M.T.	App. R.A.	Log. f. par.	App. Decl.	Log. f. par.
1872, Mar. 27	8h44m35s	11h55m41s·75	1·4501	+10°56′19″·7	0·7012

Comparison Star.

	Magnitude.	Mean R.A. for 1872·0.	Correction for Date.	Mean Decl. for 1872·0.	Correction for Date.
932 Weisse, H. 11	9	11h54m58s·51	+0s·98	+11°3′56″·85	−5″·48

The planet is of 11–12 magnitude.

Observations of the Planet (91) *Egina.*

	Marseilles M.T.	App. R.A.	App. Decl.
1872, March 13	10h30m57s	10h 02	+14°7′35″·9

Comparison Star.

	Magnitude.	Mean R.A. for 1872·0.	Correction for Date.	Mean Decl. for 1872·0.	Correction for Date.
3475 B.A.C.	6	10h4m45s·16	+0s·82	+13°59′9″·13	−2″·6

The planet is of the 11th magnitude.

New Nebulæ discovered and observed by Alph. Borrelly at the Marseilles Observatory, with the Eichens Searcher.

Mean Positions for 1872·0.

Name of Comp. Star.	R.A. h m s	N.P.D. ° ′ ″	Remarks.
a	6 39 21·87	5 26 3·7	Nebula pretty faint, extended, elliptic, no bright point.
b	6 48 44·78	4 4 20·2	Nebula pretty bright, moderately extended, round; nucleus of 12–13 mag.
c	8 51 44·10	11 25 18·2	Nebula pretty bright, 3′ in extent, elliptic; no nucleus.
d	11 37 50·45	72 43 31·6	Nebula round, little extended; small nucleus at centre.
e	11 45 26·37	72 28 20·3	Nebula pretty faint, extended, elliptic; no bright point.
e	11 45 35·87	72 25 46·1	Nebula exceedingly faint, nearly round, almost undiscernible.

Mean Positions of the Comparison Stars for 1872·0.

	Name of Stag.	Mag.	R.A. h m s	N.P.D. ° ′ ″
a	374 Œltzen	7·5	6 25 56·27	5 11 46·4
b	381 Œltzen	7·8	6 41 8·55	4 3 39·8
c	1373 Fedorenko	8·0	8 41 35·40	11 22 51·2
d	666 Weisse, hora 11	9·0	11 35 5·45	72 37 6·2
e	889 Weisse, hora 11	8·9	11 46 36·24	72 26 11·8

Variable Star.

On November 3, 1871, a star in R.A. $0^h17^m17^s{\cdot}68$ and N.P.D. $100°10'10''{\cdot}1$ (mean position for 1872·0) appeared to be of magnitude 6–7. On the 8th it was of the 8th magnitude. On the 24th I noted it as of the 10th magnitude. From November 30 until January 1872 I noticed no change. Since then I have been unable to observe it.

Discovery of Minor Planets (119) *and* (120).

A communication has been received from M. Delaunay, Director of the Observatory of Paris, announcing the discovery, by M. Paul Henry, of Paris, of Minor Planet (119). It was first observed on the 9th of April, at 11 P.M.; but the observation then obtained was only approximate. A second observation was made on the next night by M. Prosper Henry, with the equatoreal, in the garden of the Observatory of Paris.

	Paris M.T. h m s	R.A. h m s	N.P.D. ° ′ ″
1872, April 9	11 0 0	13 18 59	98 40 23
10	12 41 49	13 18 4·61	..
10	12 56 0	..	98 37 34·0

The hourly motion in R.A. is $-1^s{\cdot}75$, and in N.P.D. $-25''$ The planet is of the 11th magnitude.

The following telegram has been received by M. Delaunay from M. Borrelly, of Marseilles :—

"I have discovered this evening at 11^h, a new Minor Planet (120), the observed positions of which are as follows :—

	Marseilles M.T. h m s	App. R.A. h m s	App. N.P D. ° ′ ″
1872, April 10	12 16 32·0	12 0 55·38	95 2 44·9
10	13 14 36·5	12 0 53·63	95 2 41·4

Comparison Star.

In a letter addressed to the President, M. Borrelly gives the following additional particulars :—

	Magnitude.	Mean R.A. for 1872·0.	Correction for Date.	Mean N.P.D. for 1872·0.	Correction for Date.
994 Weisse, hora 11	7–8	$11^h59^m1^s·96$	$+0^s·93$	95°7′58″·64	+6″·73

The planet is equal in brightness to a star of 11-12 magnitude.

April 12, 1872.

Observations of Encke's Comet, at Mr. Bishop's Observatory, Twickenham. By Mr. Hind.

The observations marked H are by Mr. Hind; those marked P by Mr. William E. Plummer. The place of October 8 is uncertain from the faintness of the comet.

1871. Twickenham M.T.	h m s	R.A. h m s	Log. Fact. Par.	Decl. ° ′ ″	Log. Fact. Par.	
Oct. 8	7 41 8	1 23 2·4:	−8·6789	+35 39 9:	9·6990	H
10	10 30 42	1 15 29·1	−8·2895	36 15 36	9·4565	H
12	8 2 6	1 7 49·12	−8·6313	36 47 12·6	9·6139	P
	9 16 18	1 7 37·76	−8·4945	36 47 38·4	9·5115	H
14	8 28 24	0 58 49·29	−8·5774	37 18 32·6	9·5499	P
18	9 1 22	0 37 33·38	−8·3868	38 13 52·0	9·4005	—
Nov. 5	10 3 38	22 7 19·62	+8·5488	35 22 38·0	9·5587	H
15	6 53 21	20 32 6·89	+8·3630	25 27 51·1	9·6701	P
16	7 30 41	20 23 4·37	+8·4883	24 10 32·7	9·7177	H
17	7 6 13	20 14 36·51	+8·4504	22 54 5·3	9·7228	P
18	6 59 1	20 6 13·67	+8·4864	21 35 24·7	9·7410	—
20	7 48 52	19 49 42·28	+8·5807	18 51 54·0	9·5047	—
Dec. 2	5 4 18	18 27 19·41	+8·5067	3 3 34·8	9·8794	H
3	5 15 28	18 21 15·82	+8·5371	1 47 46·1	9·8856	—
4	5 12 5	15 23·01	+8·5463	0 33 52·6	9·6110	P
	5 27 57	18 15 20·41	+8·5630	+0 33 10·7	9·8905	H
5	5 16 4	18 9 36·76	+8·5618	−0 38 50·3	9·6336	P
6	5 17 22	18 3 57·71	+8·5728	−1 50 48·9	9·6496	—

Orbit of the Binary Star Σ 1938 *near* μ^2 *Boötis.*
By Mr. Hind.

The following elements fairly represent the measures of this star up to the date of nearest approach of the components:—

T	....	1860·88
Node	....	163°11′
λ	....	54 27
γ	....	41 52
$\varphi = \sin^{-1} e$		34 20·4
a	....	1″·761

Period of Revolution, $314^{yrs}\cdot34$

For 1865·46	Dawes	$\theta\,(c-o)$	$=-1°\cdot86$
1867·56	Engelmann		$=+1\cdot02$

In this orbit the angle and distance are thus given:—

1872·0	166°·28	0″·880
73·0	163·52	0·902
74·0	160·90	0·920
75·0	158·38	0·931
76·0	155·97	0·935

Orbit of the Binary Star ξ *Boötis.* By Mr. Hind.

Elements of this star were first given by Sir John Herschel in the *Memoirs* of this Society, and an orbit has also been computed by Mädler. The following elements appear to represent the whole course of measures better than any previously published;—

T	....	1779·75
Node	....	11°23′
λ	....	96 25
γ	....	71 36
φ	....	51 28
a	....	9″·95

Period, $168^{yrs}\cdot91$ $\log \mu_0 = -0\cdot32864$

Hence the angles of positions and distances are—

1872·0	291°·25	5″·638
73·0	289·95	5·613
74·0	288·63	5·587
75·0	287·29	5·561
76·0	285·94	5·535

The measures of *ξ Ursæ Majoris*, published in the *Monthly Notice* for March by Mr. Knott, indicate that the peri-astron passage will fall earlier than assigned in the elaborately worked orbit of M. Yvon Villarceau (*Conn. des Temps*, 1852). The mean of Mr. Knott's measures give an angle of position 33° in advance of that computed. The angle in Capt. Jacob's orbit (*Mem. R. Astron. Soc.*, vol. xvi.) is 26° behind the observed one. Observations during the next few years will have great value for the better determination of the orbit of this star, which, as being the first submitted to calculation by M. Savary, possesses an especial interest.

On the large number of Stars visible to the Naked Eye in the Southern Heavens. By Richard A. Proctor, B.A. (Cambridge.)

Dr. Gould makes the following remarks in his address at the inauguration of the Argentine Observatory at Cordoba: "The transparency of the sky of Cordoba upon favourable nights may be judged of by a single additional fact. You will find in the treatises on astronomy the total number of stars in the entire heavens visible to the naked eye estimated at from 5500 to 6000. Now, we have already recorded in the *Uranometria Argentina* the places of not less than 6400 stars, visible to the unaided sight of every one of our observers, in the Southern Hemisphere alone; while in the first 10° of the Northern Hemisphere we have 800 more, making in all at least 7200 stars; so that we are justified in the belief that were the sky equally transparent for astronomers in the Northern Hemisphere, the total number of stars visible to the ordinary eye would be estimated at certainly not less than 11,000, instead of half that number."

It seems to me, that until the contrary has been demonstrated by some independent evidence, the circumstances recorded by Dr. Gould should be held to show that the southern skies are really richer in stars than the northern. It may be remembered that in the preface to my large Star Atlas I had already indicated my belief that there is an aggregation of lucid stars in the southern heavens,—this aggregation being however local, insomuch that certain parts of the southern heavens are exceedingly poor in stars, while others are as remarkably rich. Now, if Dr. Gould finds, on examining the arrangement of the 6400 stars observed at Cordoba, that the local aggregation I have spoken of exists,—a result of which I do not entertain the slightest doubt,—it will be evident that what is in question is a true law of stellar distribution, and not the greater transparency of southern skies.

I must confess I know of no evidence whatever tending to show that, *cæteris paribus*, the skies are more transparent in the

southern than in the northern hemisphere. Sir J. Herschel, by proposing to continue his photometric experiments on the stars during the whole continuance of his voyage home from the Cape, implied that he at least recognised no such distinction. Why, indeed, should the southern skies be more transparent than the northern? It is true that in passing southwards from our northern latitudes we gain regions where the skies are purer; but would not the inhabitant of corresponding southern latitudes find a corresponding change as he travelled northwards? The low barometer of high antarctic latitudes will not account for the supposed difference, and the greater extent of ocean surface in the southern hemisphere must far more importantly tend to render the southern skies less pure.